The Committee on Mr. Van Denburgh's paper recommended its publication in the *Transactions*, and it was so ordered.

The meeting was adjourned by the presiding officer.

Special Meeting, October 11, 1897.

Vice-President PEPPER in the Chair.

Present, 41 members.

The special meeting was called by the President for the reception of communications on subjects of science.

Lord Kelvin was presented to the Chair, and took his seat in the Society.

Communications were made by Prof. Heilprin on "The Absence of Glacial Action in Northern Africa."

By Prof. G. F. Barker, on "The Constitution of Matter."

By Prof. Doolittle, on "Latitude Variation."

By Dr. Kennelly, on "A Speculation upon the Nature of Cathode Rays."

By Dr. D. G. Brinton, on "The Measurement of Thought as Function."

The meeting was adjourned by the Chair.

THE VARIATION OF TERRESTRIAL LATITUDE.

BY C. L. DOOLITTLE.

(*Read October 11, 1897.*)

As the distinguished scientist with whose presence we are favored this evening has on various public occasions shown a deep interest in the problem of latitude variation, it has occurred to me that a brief communication on this subject might not be out of place on this occasion. In this connection I shall speak of the work which we are doing at our newly erected Observatory of the University of Pennsylvania. As the audience is not composed exclusively of professional astronomers, it will perhaps be as well to give a brief statement in explanation of the problem.

The idea of possible variations in the latitude of points on the earth's surface is by no means a new one. It is, in fact, as old as Ptolemy. At various times, from that day to this, apparent changes of this character have renewed the interest of scientific men in this subject. It is probably superfluous to say that these supposed changes of latitude were almost exclusively due to imperfect methods of observation and to want of knowledge of various theoretical matters now well understood, that with improvements in instruments, with more perfect knowledge of the effect of refraction, with the discovery of aberration and nutation these supposed changes for the most part disappeared.

More than a hundred years ago, the illustrious Euler gave to this subject something like a scientific basis. In the development of the law of rotation of a rigid body, for which we are indebted to him, it was shown that a body like the earth, supposed to be perfectly rigid and in form an ellipsoid of revolution, unless originally started in its diurnal rotation about an axis exactly coinciding with the axis of figure, would have in addition to this diurnal rotation another motion. Suppose the original axis of rotation to make a small angle with the axis of figure. It was shown that this axis of rotation would itself revolve about the axis of figure, describing the surface of a cone, the angle between the two axes remaining unchanged. The period of this rotation depends upon the principal moments of inertia of the earth or upon their relation to each other, which may be found from the observed values of the constants of precession and nutation. The resulting period proves to be about 305 days, or ten months.

If, therefore, this period has a real existence, that is, if the axes of rotation and figure do not exactly coincide, it will, according to this theory, be shown by a periodic increase and diminution of all terrestrial latitudes in a corresponding period. Such a change was not found, though sought for by many investigators. In fact, the apparent perfection of the theory probably retarded the true development of the matter for several years. However, about ten or fifteen years ago, a number of apparent changes of latitude, found from various series of observations made at different places and by different methods, began to awaken a widespread interest in the subject. That the changes were real admitted of but little doubt. It was equally obvious that they could not be fitted into the ten-month period of Euler.

Finally, Mr. Seth C. Chandler, by a most laborious investigation, embracing an analysis of many thousands of observations, extending over a hundred and fifty years, succeeded in solving the mystery, at least in so far as existing evidence can solve it. The result shows that the earth's axis of rotation moves about the axis of figure not in one simple period of ten months, but in two periods of twelve and fourteen months respectively, with perhaps a third of several years. The combination of these motions results in a somewhat complicated curve. If we suppose circles drawn about the north and south poles of the earth having diameters of about fifty feet, the extremities of the axis of rotation will always be found within these circles.

For the purpose of perfecting our knowledge of these motions, for obtaining data for a correct explanation of the same from a theoretical standpoint, very accurate and carefully executed series of latitude observations at different points are necessary. It is such a series we have undertaken at the Flower Observatory. This series is a continuation as far as may be of that which was kept up for several years at South Bethlehem.

In this work the instrument employed is the Zenith Telescope. The stars observed are arranged in groups so selected that the errors in the positions of the stars are almost completely eliminated in so far as they effect the apparent change of latitude. Regular observation at our Observatory for this purpose was begun October 1, 1896. Since this date observations have been made on nearly every favorable night. Two series constitute a complete night's work, the first in the early evening soon after dark, the second in the morning before sunrise. Each series embraces ten pairs of stars, requiring about two hours of actual observation. As will be seen, the work is laborious. This with the necessary numerical computation might very well be regarded as sufficient employment for one person.

Some 1700 observations, extending from 1896, October 1, to 1897, August 26, have been reduced so that we can form some judgment of the results obtained and of the character of the work.

We find a pretty satisfactory agreement with the theoretical results as given by Chandler's formula, the range of variation being about $0''.4$, as indicated both by observation and theory. The phase, however, is a little more discordant, that is the times of maxima and minima, as shown by observation, do not quite agree with those indicated by theory. However, the amount of material is not yet sufficiently great to warrant any sweeping conclusions.

As to the quality of the work, the probable error of a single observed latitude is found to be $0''.14$. This is derived from the mean of everything, whether the conditions were favorable or otherwise. It does not take into account the errors of the star places used, but is simply what is sometimes called the internal probable error.

The probable error of the mean of one series of ten observations will be something over $''.04$. Nevertheless we find in comparing the results from consecutive evenings ranges sometimes as great as $0''.5$, or say twelve times the probable error. Such variations from evening to evening are not peculiar to our own work, but are found in every extensive series which I have examined. In some cases the range has been found as great as $0''.7$. The cause of these discrepancies is at present very much of a mystery. They are presumably due in great part to atmospheric causes, producing anomalous refraction phenomena. It is apparently a very difficult matter to deal with, but unless means can be found for doing so it would appear that we have about reached the limit of accuracy attainable in this class of work.

The instances before mentioned are the extreme ones. The fluctuations from night to night are usually far within the amounts mentioned. Usually it will be difficult to determine whether the variation is a real one or simply represents the error of observation. An unusually favorable opportunity for investigating this matter was furnished by the work of Marcuse, of Berlin, and Preston, of the U. S. Coast Survey, who, in 1891–92, carried on similar series of observations at Waikiki, in observatories separated by only a few feet. A comparison of the results with reference to this point was made by Marcuse, but the result was not very decisive. In a general way the number of cases of agreement in the direction of the variation was about twice as great as that of disagreement. One case in particular was very interesting, where for nine consecutive days the latitudes given by the two different observers always varied alike. In other cases, however, for many consecutive days no agreement between the two series is apparent. It is greatly to be desired that this matter should receive a thorough investigation, but the method by which the problem may be successfully attacked is not obvious.

THE MEASUREMENT OF THOUGHT AS FUNCTION.

BY DANIEL G. BRINTON, M.D.

(*Read October 11, 1897.*)

I can best introduce what I have to say by a quotation from the address of Vice-President McGee before the late meeting of the American Association at Detroit. He refers in it to a distinguished member of our own Society who, I am glad to see, is with us to-night; and the words I am about to quote are of such a tenor that that they cannot be otherwise than agreeable. Mr. McGee said:

"Less than a quarter of a century ago Barker was deemed bold unto recklessness for undertaking to correlate vital and physical forces, and many heads were shaken doubtfully when, in his presidential address before the American Association at Boston in 1880, the same brilliant experimentalist argued from the application of Mosso's plethysmograph that mental force also may be weighed and measured, so that it must be regarded as interconvertible with other forms of energy; yet less than half a generation of organic chemistry has established these revolutionary propositions beyond peradventure" (*The Science of Humanity*, by W. J. McGee, Vice-Pres., Address before Section H, at Detroit, 1897).

These words must have been intended by their writer to have important limitations. If taken in their ordinary sense they would convey a very erroneous idea of the achievements of physical and chemical science.

It is quite true that the action of thinking is in one sense a function of the brain, and is accompanied by cell destruction, by increased temperature and by the increased elimination of inorganic matter through the secretory organs. For this reason it was said by one of the older physiologists that "without phosphorus there is no thought." In a somewhat similar manner others have undertaken to demonstrate that thought is merely a mechanical process,